



FEDERAL SECURITY AGENCY  
PUBLIC HEALTH SERVICE

IN REPLYING, ADDRESS THE

March 6, 1953

Communicable Disease Center  
Enteric Bacteriology Laboratories  
P. O. Box 185  
Chamblee, Georgia

Dr. Joshua Lederberg  
Department of Genetics  
The University of Wisconsin  
Madison 6, Wisconsin

Dear Dr. Lederberg:

Your letter of February 28 and the cultures arrived today. Naturally, I do not want to say anything about them until I have played with them a bit. I have some ideas which probably are wrong. I have been wanting to look into the kunzendorf puzzle since you left but have not found time.

As to lability of H antigens in formalin - you will find some remarks about this in Ky. Bull. 499 (Arizona group). I have seen this property only in that group. However, I do not recall having encountered an antigen only a portion of which was susceptible to formalin - it has been an all or none thing.

Culture 191 was received from Cornell (N. Y. State Vet. College) under the name "Theill" and was said to have been isolated from a turkey. Its antigens were IV,V,XII:--1,2,3. While the 3 has been dropped from the schema, it is a weak factor possessed by S. typhi murium but not by S. paratyphi B. It would be rather surprising if 191 were S. paratyphi B, but in the absence of phase 1, one is always venturing on very thin ice to assign an organism to a given type.

The Hines V.A.H. culture came from the Veterans Administration Hospital in Hines, Illinois and was isolated from man. I have no further history and it could well be a natural reversion from a monophasic b, thus paralleling the artificially produced 157. This would be the logical assumption since monophasic b strains occur frequently, monophasic 1,2 strains very rarely. Their relative frequency is about the same as VI,VII: 1,5 and VI,VII: c strains of the kunzendorf variety. I have always considered the latter as derivatives of VI,VII: 1,5.

We should have sent you some S. bredeney strains which lack XXVII before this. I can only plead pressure of other work. I shall try to see that these go out soon. So far as I know, no other cultures of S. napoli are available. I am sorry about 2<sub>6</sub> experiments. I can think of no other phases which are particularly needed.

In addition to reprints of your papers in my files, and the ones you so kindly sent and are sending, the only one that I believe I can use would

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be No. 22 (Zelle and Lederberg, J. Bact., 1951, 61, 351-355).  
Probably I should have this but if so it has been loaned or misplaced.

I have been reading some of the reprints you sent. Others throw me for such a complete loss that I give up very quickly. However, I believe I'm getting a layman's view of the work. At least I understand much more than I did a month ago.

I shall send such reprints that you wanted as are available.

With kind regards, I am

For the Officer-in-Charge, Bacteriology Section

Sincerely yours,

*Phil*

PRE:mg

Philip R. Edwards, Ph. D.  
Bacteriologist-in-Charge  
Enteric Bacteriology Unit